surface outflow, he could trace an underflow of sea-water up the channel; and this he could attribute to nothing else than the slight excess of *downward* and therefore *lateral* pressure in the *outside* column, depending on the continually-maintained reduction in the mean salinity of the *inside* column, which more than compensated for any slight excess in its level.

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The Freshwater Medusa

IN NATURE (vol. xxii. p. 190) Prof. Lankester refers to a statement of mine in the preceding number, that I had arranged with Mr. Sowerby some methods of observation from which I hoped to obtain data for the determination of important points regarding the development of the freshwater Medusa, and expresses a desire to be informed as to the nature of the proposed methods.

The obvious and only practicable course to be adopted with this view was arranged with Mr. Sowerby by Mr. Busk and myself, and consisted in the separation of specimens from the Victoria tank and their confinement in glass jars, which, in order to secure a continuance of the necessary temperature conditions, were to be retained in the same house with the tank in which the Medusa had shown itself. The examination from time to time of these jars would probably bring to light facts having a direct bearing on the development of the animal. This method of observation, indeed, is so obvious that it must have occurred to any one engaged in the investigation it was designed to aid.

Prof. Lankester now says that Mr. Sowerby informs him that he had undertaken no experiments except such as had been carried out at his request; but as it seems that these are identical with those proposed by Mr. Busk and myself, nothing has been thereby lost.

Residing at a distance from London, my opportunities of studying the life-history of the Medusa are at this moment comparatively few. Prof. Lankester, however, being on the spot, and having an unlimited supply of subjects for investigation, will doubtless avail himself of the advantage thus afforded, and will render our knowledge of this remarkable little animal more complete than would otherwise have been possible.

Prof. Lankester refers to the difference of opinion between himself and me, and promises to bring proofs of his own views. When these proofs are offered I shall gladly accept them. My desire is that no previous expression of opinion shall blind me to evidence in favour of a contrary position. The only important points, however, on which my conclusions have been absolutely at variance with those of Prof. Lankester are the presence of a circular canal and the perviousness of the distal extremities of the radial canals. With regard to these there cannot in my opinion be the slightest doubt.

The nature of the marginal bodies is also a point of much importance in this investigation, but I have expressed only a conditional opinion with regard to it. While Prof. Lankester considered these bodies as undoubtedly tentacular, I held that the evidence afforded by adult and by comparatively young specimens is in favour of their velar origin; but at the same time I stated that this point cannot be decided without the evidence obtained from development.

I also drew attention to the remarkable attachment of the tentacles, whose adnate basal portion occupies exactly the position of the *peronia* in the Narcomedusæ and Trachomedusæ, but I failed to find evidence of the presence of true peronia as described by Prof. Lankester, who now admits that the peronia while present are rudimental.

The other points, namely those which concern the systematic position of the Medusa, are necessarily only hypothetical. It appeared to me that while there are certain features in the structure of the adult Medusa which point towards the Trachomedusæ, there are others which connect it with the Leptomedusæ, to which on the whole it seemed to be more closely allied, though holding a position intermediate between the two; but I regarded the data in our possession as insufficient for the final determination of this point, which can be absolutely settled by the study of development alone.

Prof. Lankester promises details of his observations in this month's number of the *Quarterly Journal of Microscopic Science*, and I look forward to what I doubt not will be a valuable contribution to hydroid zoology.

As to the name of the Medusa, Prof. Lankester, while abandoning his generic name in favour of mine, declares it to be his intention to retain his own specific name for the animal. This is to me a matter of complete indifference. Science can gain nothing from personal contention about names, and the time so occupied might with far greater advantage be devoted to more useful and lasting work.

J. ALLMAN

On the Simplest Continuous Manifold of Two Dimensions and of Finite Extent

So far as I am concerned Mr. Frankland answers too soon (p. 170), for I am sorry to say I have not read Klein in the meantime. Therefore my reply is provisional. A hint was given of Mr. Frankland's explanation by Mr. Newcomb in a phrase quoted by Mr. Halsted (*American Journ. of Math.*, I. iii. 275, paper on the bibliography of hyperspace, &c.): "The first elements of complex functions imply that a line can change direction without passing through infinity or zero." We do not require even the first elements of complex functions to tell us that we can get to the other side of a point without passing through it, provided we can go round it. But the question was not whether "a line" simply could be thus reversed, but whether it could be so with the geodetic perpendicular in question described in a uniform continuous manifold of two dimensions. Mr. Frankland's explanation expressly takes account of a third dimension. It supposes the moving line to generate a sort of skew helicoid about the fixed line to which it is perpendicular. But how can even initial portions of successive generators be in the same plane, Euclidean or other? This point may seem incidental, but I think it is essential, so I omit further questions.

Somewhere in his "Dynamic" Clifford says that Klein's double surface is a sphere in which opposite points are considered as one. In this light the mystery disappears. There are two perpendiculars: considered as one they never change sign; because, considered as two, they periodically exchange signs. But if opposite points do not coincide, they may be "one," but they are not one point; if they do, is the manifold they compose a surface? Mr. Frankland has not called it a surface: but is it continuous?

There is a very well-known manifold which obviously obeys the laws worked out by Mr. Frankland and Mr. Newcomb, a system of straight lines, not vectors, through a common point; or, reciprocally, a system of planes. To measure of curvature answers density; if this is constant, the geodetic distance from a point to a geodetic line is represented by the angle between a straight line and a plane.

It may be worth while to note one or two oversights in the writing or printing of Mr. Frankland's letter. For $\frac{1}{2}l\sqrt{-1}$ we ought to have an expression involving the angle between the geodetics. The sentence "If a being," &c., is a quotation, and the last word should be "position," not "poise."

Both Mr. Newcomb and Mr. Frankland understand my intention as more negative than it was. I said (xv. 547) "it could hardly fail to be instructive if Mr. Frankland would explain," &c. Probably I underrated the difficulty, in this Euclidean world, of making it clear that one means just what one says.

C. J. MONRO

Hadley, June 29

A Fourth State of Matter

IT seems to me that Mr. Tolver Preston in his letter on the above to NATURE (vol. xxii. p. 192) has somewhat overlooked the context in the objections he urges against Mr. Crookes's remark that "an isolated molecule is an inconceivable entity." It is plain that Mr. Crookes meant this statement to apply to the *quality*, not the *existence* of a molecule, and granting Mr. Crookes's premisses regarding the constitution of matter, it appears a very fair deduction; since if the three states of matter (as we know it), viz., solid, liquid, and gas, owe their different *qualities* merely to different modes of motion of the ultimate molecules, it is quite conceivable as well as logical to suppose that the latter have a nature totally unlike that of the effects of their motion, and therefore inconceivable to us by reason of its dissimilarity to anything of which we at present possess any knowledge.

Again, with reference to the remark, "solid it cannot be,"

which Mr. Preston calls in question, it would be manifestly illogical on his premisses for Mr. Crookes to regard the isolated molecule as a solid, even though, according to Mr. Preston, it may possibly possess certain properties in common with what we call solids, for solidity, according to Mr. Crookes, being "merely the effect on our senses of the motion of the discrete molecules among themselves," it would be exceedingly arbitrary to ascribe to the molecules themselves a quality which, as we *commonly* know it, is simply an *effect* of their motion.

July 3 E. DOUGLAS ARCHIBALD

Minerva Ornaments

I NOTICE that a correspondent writing from America expresses his scepticism as to the figural character of certain stone objects in Dr. Schliemann's collection at South Kensington. Judging from the analogy of similar objects found in America, he pronounces them to be "net-sinkers" and not idols. Whatever, however, may be the nature of the American objects, I think there can be but little doubt that Dr. Schliemann is right in considering the objects discovered by him at Hissarlik to be rude representations of a deity. At first sight they certainly have but little resemblance to anything of the sort, but a careful examination shows that several are marked with the rude delineation of a human face—or, as Dr. Schliemann believes, of an owl's face—as well as of a triple necklace, and sometimes also the characteristics of a woman. Occasionally the hair is represented on the back of the head by straight lines. The delineation is sometimes incised, sometimes painted, though the paint is mostly worn off. As the marked objects are of the same shape as the unmarked ones, we can have no hesitation in inferring that both were intended for the same purpose. A. H. SAYCE

July 4

Arthur Young's Travels in France

A FEW months ago my friend Mr. F. F. Tuckett, of Bristol, drew my attention to a passage in Arthur Young's Travels in France, published in 1792, narrating a visit to Lavoisier and to a certain M. Lomond, the inventor of an electric telegraph, which in some points anticipated that of Ronalds. The mention of Lomond's name in a historical list of telegraphic inventors recently published by your contemporary, the *Scientific American*, induces me to send you the inclosed extract as likely to be of interest to the readers of NATURE. S. P. THOMPSON

Univ. Coll., Bristol, June 18

"The 16th.—To M. Lavoisier by appointment. Madame Lavoisier, a lively, sensible, scientific lady, had prepared a *déjeuné Anglois* of tea and coffee, but her conversation on Mr. Kirwan's Essay on Phlogiston, which she is translating from the English, and on other subjects which a woman of understanding, that works with her husband in his laboratory, knows how to adorn, was the best repast. That apartment, the operations of which have been rendered so interesting to the philosophical world, I had pleasure in viewing. In the apparatus for aërial experiments nothing makes so great a figure as the machine for burning inflammable and vital air, to make or deposit water; it is a splendid machine.

"Three vessels are held in suspension with indexes for marking the immediate variations of their weights; two, that are as large as half-hogsheads, contain the one inflammable, the other the vital air, and a tube of communication passes to the third, where the two airs unite and burn; by contrivances, too complex to describe without plates, the loss of weight of the two airs, as indicated by their respective balances, equal at every moment to the gain in the third vessel from the formation or deposition of water, it not being yet ascertained whether the water be actually made or deposited. If accurate (of which I must confess I have little conception) it is a noble machine. Mons. Lavoisier, when the structure of it was commenced, said, 'Mais oui, monsieur, et même par un artiste François!' with an accent of voice that admitted their general inferiority to ours. It is well known that we have a considerable exportation of mathematical and other curious instruments to every part of Europe, and to France among the rest. Nor is this new, for the apparatus with which the French Academicians measured a degree in the polar circle was made by Mr. George Graham. Another engine Mons. Lavoisier showed us was an electrical apparatus inclosed in a ballcon, for trying electrical experiments in any sort of air. His pond of quicksilver is considerable, containing 250 lbs., and his water apparatus is great, but his furnace did not seem so well calculated for the higher degrees of heat as some others I have seen. I was glad to find this gentleman splendidly lodged and with every appearance of a man of considerable fortune. This ever gives one pleasure: the employments of a state can never be in better hands than of men who thus apply the superfluity of their wealth. From the use that is generally made of money, one would think it the assistance of all others of the least consequence in affecting any business truly useful to mankind, many of the great discoveries that have enlarged the horizon of science having been in this respect the result of means seemingly inadequate to the end: the energetic exertions of ardent minds, bursting from obscurity, and breaking the bonds inflicted by poverty, perhaps by distress.

"To the 'Hotel des Invalids,' the major of which establishment had the goodness to show the whole of it. In the evening to Mons. Lomond, a very ingenious and inventive mechanic, who has made an improvement of the jenny for spinning cotton. Common machines are said to make too hard a thread for certain fabrics, but this forms it loose and spongy.

"In electricity he has made a remarkable discovery: you write two or three words on a paper, he takes it into a room and turns a machine inclosed in a cylindrical case, at the top of which is an electrometer, a fine small pith ball; a wire connects with a similar cylinder and electrometer in a distant apartment; and his wife, by remarking the corresponding motions of the ball, writes down the words they indicate: from which it appears he has found an alphabet of motions. As the length of the wire makes no difference in the effect, a correspondence might be carried on at a distance—within and without a besieged town, for instance, or for a purpose much more worthy, and a thousand times more harmless, between two lovers prohibited or prevented from any better connection.

"Whatever the use may be, the invention is beautiful. Mons. Lomond has many other curious machines, all the entire work of his own hands. Mechanical invention seems to be in him a natural propensity." ("Travels during the Years 1787, 1788, and 1789," by Arthur Young, Esq., F.R.S. Vol. i. p. 64.)

"Saxifraga umbrosa" adorned with Brilliant Colours by the Selection of Syrphidæ

AMONG Diptera the most assiduous visitors of flowers are certain Syrphidæ, which, elegantly coloured themselves, are fond of splendid flower-colours, and, before eating pollen or sucking nectar, like to stop a while, hovering free in the air, in front of their favourites, apparently fascinated, or at least delighted, by the brilliancy of their colours. Thus I repeatedly observed *Syrphus balteatus* hovering before the flowers of *Verbascum nigrum*, often *Melanostoma mellina*, and *Ascia podagrica* before *Veronica chamædrys*; in the Alps the lank *Sphegina clunipes* before *Saxifraga rotundifolia*, and in my garden *Ascia podagrica* before *Saxifraga umbrosa*.

Of *Verbascum nigrum* the main fertilisers are humble-bees, Diptera co-operating only in a subordinate degree; in the case of the three other species, on the contrary, the above-named Syrphidæ are such frequent visitors and cross-fertilisers that we may safely conclude that it is by their selection of elegantly-coloured varieties that these flowers have acquired their beautiful peculiarity. Hence, in order to estimate the colour-sense of these Syrphidæ, it is worth while to consider what colour-combinations they have been able to produce by their selection.

Saxifraga umbrosa being, as far as hitherto known, their finest masterpiece, we may in the first place look at the variegated decoration of this species. Its snow-white petals are adorned with coloured spots, which in size and intensity of light gradually decrease from the base of the petals towards their extremity. Indeed, nearest to their base, within the first third of their length, there is a large irregular spot of an intense yellow; about the middle of their length there follows a narrower cross band of red colour, vermilion towards the base, intensely pink towards the outside, not reaching the margins of the petals, sometimes dissolved into several separate spots; lastly, beyond the middle of the length of the petals there are three to eight smaller roundish spots of a paler violet-pink colour.

The flowers of *Veronica chamædrys* prove that also gay blue colours are perceived and selected by Ascia.

Lippstadt, Germany HERMANN MÜLLER